Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

XVIII.

ON A POSSIBLE EXPLANATION OF THE METHOD EMPLOYED BY NOBERT IN RULING HIS TEST PLATES.

BY WILLIAM A. ROGERS.

Presented, June 9, 1875.

I RECOGNIZE the fact that no explanation of a purely mechanical process can be regarded as either satisfactory or final, which does not answer the crucial test of reproduction. I offer to the Academy what I believe may prove to be an explanation of the process followed by Nobert in ruling his test plates; the highest band which has been resolved under the microscope, reaching 112,600 lines to the inch. You properly ask me if I can reproduce these rulings. I frankly answer that I cannot. Indeed, I can hardly hope ever to succeed in producing lines which combine the wonderful delicacy, uniformity, and distinctness found in nearly all of Nobert's plates. But I have reached what I hope may prove to be a useful approximation to Nobert's results. Beginning with 2000 lines to the inch in 1871, I have now little difficulty in reaching 60,000, the width of each line being a little less than one half of the intervening space. Several of my plates have been correctly counted as far as 80,000 to the inch; the observer having no knowledge of the actual number ruled. Two plates in the possession of Frederick Habirshaw, Esq., of New York, contain bands proceeding by 10,000 as far as 120,000 to the inch. The bands of both these plates were correctly counted by Samuel Wells, Esq., of Boston, as far as 80,000, but beyond that point the number counted was less than the number ruled. While the lines of the higher bands seem to be nearly as distinct as Nobert's, they are by no means as smooth and uniform throughout their whole length.

The theory which I offer to the Academy is wholly the outgrowth of my own experience. In the various experiments which I have made, I have noted the constant recurrence of certain results under certain conditions, and these results form the basis of my conclusions. Whether they form a true explanation of Nobert's process is, of course, entirely a matter of conjecture. I am well aware of the risk incurred in offering a theory which can at once be refuted by a single stroke of the pen. Nobert has well kept the secret of his process. If I have failed to detect it, it is easy for him to say, " You are wholly mistaken." Even if this proves to be the case, the facts developed in the course of my experiments may possess sufficient interest to warrant their publication.

The problem is naturally divided into two parts : —

(*a*). The mechanical operation of moving the plate to be ruled over given and equal spaces.

(*b*). The operation of producing on glass, lines of varying degrees of fineness.

If a screw is employed to give the required motion, it would seem at first sight very easy to reach any desired limit of accuracy. In my own machine, the head of the screw, which is 11 inches in diameter, is divided into 100 equal parts. For subdivisions, a microscope is employed, having an eye-piece micrometer, 100 divisions of which exactly cover one division of the screw-head. It is therefore easy to read directly to $\frac{1}{10000}$, and by estimation to $\frac{1}{40000}$ of a revolution. Since the pitch of the screw is $\frac{1}{24}$ of an inch, these numbers correspond to a motion of $\frac{1}{240000}$ and $\frac{1}{960000}$ of an inch. By a device which I shall presently describe, the subdivisions can be carried to $\frac{1}{1000000}$ of a revolution.

But nothing can be farther from the truth than to suppose that, because this high limit of theoretical accuracy can be reached, therefore the lines ruled are separated by spaces accurate within the same limits. It is difficult to name the lowest limit of deviation from the truth which it is possible to reach ; but I have long since despaired of being able to rule, e.g. 100 lines, covering successive revolutions of the screw which shall contain no errors of any kind, whether individual or accumulated, greater than $\frac{1}{30000}$ inch. I have availed myself of every opportunity to measure the ordinary stage micrometers furnished by dealers in microscopes, and I find the usual range of error to be between $\frac{1}{2000}$ and $\frac{1}{10000}$ of an inch.

Of course the average error may fall far within these limits ; and, especially when the lines are closely ruled, the individual errors may seem by comparison insignificant ; but I have been unable to find *any* rulings which invariably surpass the limit which I have named. As an illustration of the limit of accuracy attainable, I give in Tables I., II., and III., measurements of an excellent Nobert diffraction plate, a

Rutherford diffraction plate, and a plate ruled by myself for the purpose of investigating the errors of my screw. The measures were all made with a $\frac{1}{15}$ objective, and an eye-piece micrometer, 200 to the inch, the lines of which were about $\frac{1}{18000}$ of an inch in breadth. Using a B. ocular, the value of one division was found to be $\frac{1}{15200}$ of an inch. With this arrangement, it was found easy to measure any given space to $\frac{1}{500000}$ of an inch with considerable certainty. To eliminate errors in the micrometer, the same divisions were used in all comparisons. In the Nobert plate, the width of the lines is about $\frac{1}{18000}$ of an inch; in the Rutherford plate, $\frac{1}{35000}$ of an inch; and in my own, $\frac{1}{28000}$ of an inch. The space measured was $\frac{1}{240}$ of an inch.

Table I. contains the residuals obtained by subtracting the measure of each space from the mean of all the spaces.

Table II. contains the residuals obtained by subtracting the error of each space from the error of the next consecutive space.

Table III. contains the periodic errors deduced from Table I.

TABLE I.

RESIDUALS FROM THE MEAN.

Spaces.	I. Nobert.	II. Rutherford.	III. Rogers.
	Fraction of inch.	Fraction of inch	Fraction of inch.
1	$+\frac{1}{21700}$	$-\frac{1}{33800}$	$-\frac{1}{44700}$
2	$-\frac{1}{76000}$	$+\frac{1}{27600}$	$-\frac{1}{44700}$
3	$+\frac{1}{38000}$	$-\frac{1}{30700}$	$-\frac{1}{23800}$
4	$-\frac{1}{76000}$	$-\frac{1}{101000}$	$-\frac{1}{34500}$
5	0	$-\frac{1}{27600}$	$-\frac{1}{34500}$
6	$+\frac{1}{38000}$	$-\frac{1}{43400}$	$-\frac{1}{44700}$
7	$+\frac{1}{16900}$	$-\frac{1}{33800}$	$-\frac{1}{108600}$
8	$-\frac{1}{21700}$	$-\frac{1}{101000}$	$-\frac{1}{380000}$
9	$-\frac{1}{152000}$	$+\frac{1}{307000}$	$+\frac{1}{253000}$
10	$-\frac{1}{38000}$	$+\frac{1}{101000}$	$+\frac{1}{27200}$
11	$-\frac{1}{50700}$	$+\frac{1}{33800}$	$+\frac{1}{27200}$
12	$+\frac{1}{76000}$	$-\frac{1}{33800}$	$+\frac{1}{20000}$
13	$+\frac{1}{152000}$	$+\frac{1}{307000}$	$+\frac{1}{33100}$
14	$-\frac{1}{76000}$	$-\frac{1}{101000}$	$+\frac{1}{42200}$
15	$+\frac{1}{19000}$	$+\frac{1}{43400}$	$+\frac{1}{57500}$
16	$+\frac{1}{50700}$	$-\frac{1}{60800}$	$+\frac{1}{23100}$
17	$-\frac{1}{19000}$	$-\frac{1}{307000}$	$-\frac{1}{108600}$
18	$-\frac{1}{30400}$	$-\frac{1}{307000}$	$-\frac{1}{380000}$
19	$-\frac{1}{50700}$	$+\frac{1}{43400}$	$-\frac{1}{34500}$
20	$+\frac{1}{152000}$	$+\frac{1}{43400}$	$-\frac{1}{44700}$

TABLE II.

CONTIGUOUS ERRORS.

Spaces.	I. Nobert.	II. Rutherford.	III. Rogers.
	Fraction of inch.	Fraction of inch.	Fraction of inch.
1	$\frac{1}{16900}$	$\frac{1}{15200}$	$\frac{1}{50700}$
2	$\frac{1}{25300}$	$\frac{1}{25300}$	$\frac{1}{76000}$
3	$\frac{1}{25300}$	$\frac{1}{152000}$	0
4	$\frac{1}{76000}$	$\frac{1}{21700}$	$\frac{1}{152000}$
5	$\frac{1}{38000}$	$\frac{1}{16900}$	$\frac{1}{76000}$
6	0	$\frac{1}{152000}$	$\frac{1}{152000}$
7	$\frac{1}{30400}$	$\frac{1}{50700}$	$\frac{1}{152000}$
8	$\frac{1}{9500}$	$\frac{1}{76000}$	$\frac{1}{30400}$
9	$\frac{1}{25300}$	$\frac{1}{152000}$	0
10	$\frac{1}{50700}$	$\frac{1}{50700}$	$\frac{1}{76000}$
11	$\frac{1}{152000}$	$\frac{1}{16900}$	$\frac{1}{50700}$
12	$\frac{1}{30400}$	$\frac{1}{30400}$	$\frac{1}{152000}$
13	$\frac{1}{15200}$	$\frac{1}{76000}$	$\frac{1}{152000}$
14	$\frac{1}{50700}$	$\frac{1}{30400}$	$\frac{1}{38000}$
15	$\frac{1}{15200}$	$\frac{1}{25300}$	$\frac{1}{19000}$
16	$\frac{1}{30400}$	$\frac{1}{50700}$	$\frac{1}{152000}$
17	$\frac{1}{13800}$	0	$\frac{1}{38000}$
18	$\frac{1}{50700}$	$\frac{1}{76000}$	$\frac{1}{152000}$
19	$\frac{1}{76000}$	0	0

TABLE III.

PERIODIC ERRORS.

Spaces.	I. Nobert.	II. Rutherford.	III. Rogers.
	Fraction of inch.	Fraction of inch.	Fraction of inch.
1	$+\frac{1}{21700}$	$-\frac{1}{33800}$	$-\frac{1}{44700}$
2	$+\frac{1}{30400}$	$+\frac{1}{152000}$	$-\frac{1}{22400}$
3	$+\frac{1}{16900}$	$+\frac{1}{30400}$	$-\frac{1}{11500}$
4	$+\frac{1}{21700}$	$-\frac{1}{152000}$	$-\frac{1}{8700}$
5	$+\frac{1}{21700}$	$+\frac{1}{33800}$	$-\frac{1}{6900}$
6	$+\frac{1}{13800}$	$+\frac{1}{152000}$	$-\frac{1}{6000}$
7	$+\frac{1}{7600}$	$-\frac{1}{43400}$	$-\frac{1}{5700}$
8	$+\frac{1}{11500}$	$-\frac{1}{30400}$	$-\frac{1}{5600}$
9	$+\frac{1}{12700}$	$-\frac{1}{33800}$	$-\frac{1}{5700}$
10	$+\frac{1}{19000}$	$-\frac{1}{50700}$	$-\frac{1}{7200}$
11	$+\frac{1}{30400}$	$+\frac{1}{100300}$	$-\frac{1}{9900}$
12	$+\frac{1}{21700}$	$-\frac{1}{50700}$	$-\frac{1}{19500}$
13	$+\frac{1}{19000}$	$-\frac{1}{60800}$	$-\frac{1}{47000}$
14	$+\frac{1}{25300}$	$-\frac{1}{38000}$	$+\frac{1}{380000}$
15	$+\frac{1}{10900}$	$-\frac{1}{30400}$	$+\frac{1}{50700}$
16	$+\frac{1}{8900}$	$-\frac{1}{76000}$	$+\frac{1}{15800}$
17	$+\frac{1}{16900}$	$-\frac{1}{60800}$	$+\frac{1}{18500}$
18	$+\frac{1}{38000}$	$-\frac{1}{50700}$	$+\frac{1}{19500}$
19	$-\frac{1}{152000}$	$-\frac{1}{100300}$	$+\frac{1}{44700}$
20	0	0	0

It is quite evident that in the three cases under consideration, there are numerous accidental errors amounting to $\frac{1}{30000}$ of an inch and more, while in the last case the evidence of periodicity is very decided; its value at the maximum point being $\frac{1}{5600}$ of an inch. An examination of the values in Tables I. and II., column III., will show how easy it is to be misled by a seeming accuracy when only consecutive spaces are measured. It is only when the errors become magnified by successive increments that they attract attention.

The following will be found a very convenient and accurate method for measuring directly the magnitude of the periodic errors.

First, a series of equidistant lines is ruled on thick glass, care being taken to use glass having a plane surface. It is better also to have the spaces correspond to equal parts of a revolution of the screw. On one side a heavy finding line is ruled. This band is then reproduced on microscopic cover glass, having a thickness of about $\frac{1}{100}$ of an inch. Of course care is taken to use the same part of the screw, and the same divisions of a revolution as before. By cementing the glasses together with balsam, face to face, but with the finding lines coincident and on opposite sides, the periodic errors, if they exist, will appear under the microscope with twice their real magnitude.

In this way it is easy to measure not only the maximum value, but the values corresponding to every division of the screw-head. If an objective of high power is employed, care is necessary to have the surfaces of both pieces of glass as nearly plane as possible. Much better results are obtained by using a piece of cover-glass not larger than $\frac{1}{16}$ of a square inch.

Mr. John M. Blake, of New Haven, did me the kindness to photograph on cover-glass, the plate whose measures are given in column III. By reversing the plates, in the way indicated above, he found almost precisely the same value for the maximum periodic error deduced above; viz., $\frac{1}{5600}$ of an inch.

In passing, it may be interesting to note that though the lines of the Rutherford plate are more distinct than those of the Nobert plate, and though the errors of spacing are considerably less, yet the former was rejected from the start as an imperfect one, while the latter gives excellent results, yet both plates will show with about equal distinctness four lines between the components of the magnesium line *b*. This is hardly in accordance with the theory that the optical test of parallelism of lines, and of equality of spacing, is far more perfect than the test of actual measurement. It is evident that the theoretical limit of accuracy required, in order to produce the solar lines in the greatest perfection, has rarely if ever been reached in actual practice. All the evidence seems to point to the conclusion that the brilliancy of the spectrum depends as much on the character of the lines, and especially on the character of the edges, as on the equality of the spacing.

It is obvious, then, that the errors which are to be the most feared, both on account of their magnitude and the likelihood of their escaping detection, are those which are periodic in their character. To the investigation of the sources of these errors, in my own machine, several months of careful study have been given. Without entering into a detailed account of fruitless experiments, I will give only the conclusion at which I have arrived; viz., *that the periodicity resides, not in the screw itself, but in the mounting of the screw.* The evidence on this point seems to be conclusive. In a large number of separate measurements extending over several weeks, substantially the same system of values as those given in column III. were found. These values were also constant for different parts of the screw. Conjecturing that the trouble might arise from unequal friction between the screw and the nut at different parts of the revolution, owing to the want of parallelism, between the screw and the fixed way on the bed of the machine, a slight movement was given to the adjusting screws, which clamped the split nut. At once the system of corrections was wholly changed, not only in value but in sign, and the values now found, remained constant under every variety of tests applied. After a few weeks, a slight movement was given to the screws holding the plate against which the precision screw works as a shoulder. The sign of the errors was again changed, but their magnitude was very much reduced, amounting at the maximum to about $\frac{1}{22000}$ of an inch. This system of errors also remained, as long as no further changes were made.

Having definitely found by these and several other similar experiments that the periodicity was not due to the precision screw itself, but to the constrained motion caused by unequal friction between the nut, the screw, and the ways on which the gravity slide, which carries the plate to be ruled, is moved, I addressed myself to the task of removing as far as possible this source of error. While I have not succeeded with entire satisfaction, the errors of a periodic character have been so much reduced that those which still remain give no serious trouble. By a device to be presently described, these residuals are overcome by an automatic movement connected with the screw itself. Omitting an account of many fruitless trials, I describe the following permanent changes which were finally made.

(*a*) The ways over which the gravity slide moves, one of which was at first ∧ shaped, and the other plane, and both of which were permanently fixed, were both made ∧ shaped and both movable. The ends nearest the point where the bearing of the shaft of the screw works against its shoulder, were pivoted. The other ends were made adjustable with set screws. The precision screw being set in its normal position, and attached to the slide by its nut, the ways are set parallel with the screw by the motion of the slide upon them.

(*b*) The nut, which at first was only about one inch long, was made four inches in length, being one half the length of the screw. About equally good results were obtained with a lead and a brass nut. The lead nut is much the more difficult to make, as a tap cannot be used. Even when it was cut with a chaser on the lathe, it was found impossible to get a smooth thread until the very simple remedy of keeping the interior wet with a strong soap lye was tried.

The nut having been fitted to the screw, the threads were reduced to a homogeneous system, and at the same time polished, by grinding with the finest emery. It should be remarked that the screw was originally finished in this way, using coarser emery at first. The rule adopted was to grind the screw till all tremor perceptible to the touch in the passage of the nut over the entire length of the screw disappeared.

(*c*) In order to set the screw parallel to the ways in a vertical direction, a hollow cylinder was firmly attached to the under side of the gravity slide. The screw, with the nut upon it, and passing through this cylinder was first set in position. The gravity slide having been firmly clamped down upon the ways, the open space surrounding the nut was then filled with plaster of Paris.

In this way the screw is set in perfect adjustment for one position of the gravity slide. Practically, it is found to be in good adjustment for every position upon the ways. But any slight deviation from adjustment in a horizontal direction is corrected by means of the adjustable ways, while that in the vertical direction is for the most part overcome by leaving one end of the precision screw free.

Good results have also been obtained by using a "free nut." In this case nice adjustments are unnecessary, as the nut moves freely upon the screw, pushing the gravity slide before it. If this arrangement is adopted, care should be taken that the nut, if not symmetrical with respect to the screw, should fall freely in the direction of gravity, and bear at every point throughout its whole length against whatever holds it in position while the screw is in action. The most serious objection to this arrangement is a certain amount of lost motion, which seems inevitable.

It is not to be inferred that all periodic errors have been overcome by the arrangement described above; but experience has shown that they have been very much diminished. In fact, I have never succeeded in ruling but two precisely similar plates, in which there was an exact coincidence of every line from beginning to end, when examined under the microscope. In one plate of 100 lines, ruled with great care, each interval being $\frac{1}{2400}$ of an inch, there are, according to three independent measures made by different persons, 84 cases in which the errors are less than $\frac{1}{100000}$ of an inch, and the greatest individual error is $\frac{1}{47500}$ of an inch; but the maximum periodic error varies with the different observers between $\frac{1}{200000}$ and $\frac{1}{500000}$ of an inch.

Nobert's bands proceed by increments of 5630 lines to the English inch. The following table gives the number of lines to the inch in each band.

Band.	Lines in an inch.	Band.	Lines in an inch.
1	11259	11	67556
2	16889	12	73186
3	22519	13	78816
4	28148	14	84445
5	33778	15	90075
6	39408	16	95705
7	45037	17	101334
8	50667	18	106964
9	56297	19	112594
10	61926		

How are these Lines accurately spaced?

The ordinary way is to give to the head of the screw, which carries the plate to be ruled, the desired movement over equal intervals by means of a ratchet and pall: but this method is open to the two objections, that one is limited to the number of teeth cut on the disc, or to an even combination of them; and, also, that all errors of the gear-cutter with which the ratchet was originally cut are transferred directly to the rulings, with the addition of other errors arising from want of centring, &c.

I have employed for this purpose the following device, which, as far as I am aware, is new in its application: A rigid arm two feet in length vibrates upon a shaft set exactly in a line with the precision screw. At one end a magnet, fitted to the curvature of the head of the screw, is attached by eight pivots in such a way as to give parallel motion with respect to the arm. The outer portion of the head of the screw consists of a rim of soft iron, which operates as an armature to draw the magnet to it when the circuit is completed. The other end of the arm works between two stops, one of which is adjustable. The action, then, is this: the circuit being completed, the magnet becomes firmly attached to the head of the screw, and by the movement of the arm from one stop to the other, it is carried over a given space. The circuit being broken, the arm during the reverse movement carries the magnet with it without disturbing the precision screw. In order to guard against every possibility of disturbance, a second magnet holds the head of the screw in place while the first one is moving back to prepare for the next increment of motion. By varying the distance between the stops, any desired motion whatever, within certain limits, can be given to the screw. From repeated experiments, it is found that about twenty movements of the arm for $\frac{1}{10000}$ of a revolution of the screw-head can be made without varying more than one from this number.

If now the lower stop is replaced by a wheel made to revolve simultaneously with the head of the screw, and if to the periphery of this wheel a curvature is given corresponding to the known errors of the screw, it is obvious that the screw can be made to correct its own errors. Thus, if at any point in its revolution the screw gives too small intervals, the periphery of the wheel must be filed away enough to increase the space ruled by the amount of the error. I am indebted to Professor Joseph Winlock, the late Director of Harvard College Observatory, for the suggestion of this elegant method of overcoming the residual errors of the screw.

How are Nobert's Lines of Varying Degrees of Fineness ruled on Glass?

First of all, the evidence seems quite clear that they are ruled with a diamond having a knife-edge. In all of the cases which I have examined the lines start in with a curvature, which is maintained throughout the whole extent of the band. I have been able to produce this result only by setting the cutting edge of the diamond slightly inclined to the direction of the line ruled, and this inclination seems to give a decided improvement to the character of the lines.

I assume that Nobert uses a prepared diamond, instead of a natural crystal. It is everywhere assumed by writers on the subject, that only the natural crystal possesses perfect cutting qualities. While this is probably true where a deep cut is wanted for the purpose of fracture, it does not seem to be true where distinct, smooth, and uniform lines are desired. I believe this is also the experience of Mr. Rutherford, who long ago abandoned the natural crystal, either unbroken, or broken into chance fragments. A circular point is objectionable for several reasons, mainly on account of its lack of durability.

Starting with the theory that Nobert's lines are ruled with a highly polished knife-edge diamond, I had constructed from my own designs an apparatus for preparing diamonds in this way.

The machine does not differ from the ordinary tool of the lapidary, except in two particulars; but these are vital to success. It is well known that diamonds can be ground and perfectly polished only in the direction of the cleavage planes, of which there are twenty-four in every perfect stone. A skilful diamond-worker will locate the position of these planes by simple inspection. I found myself obliged to employ the more tedious, but not less sure, method of finding them by a tentative process. The machine was therefore so constructed that the direction of the cleavage planes could be detected after a few trials.

Again, it is customary either to press the lap, on which the diamond dust is placed, up against the diamond, which is set in a rigid holder, or else to connect the holder to a rigid shaft by means of an intervening flat spring. In either case, the diamond is liable to crumble when it is reduced to a sharp edge. In the arrangement adopted, the holder containing the diamond is *free in the direction of gravity only.* This action is secured by two shafts set at right angles, and connected with the required supports by three universal joints. By weighting the horizontal arm or by lifting it with a spiral spring, the pressure can be regulated with great nicety. The lap has a circular movement, while the frame in which it rests has two motions in a horizontal plane, at right angles to each other. In order to give a motion in revolution to the holder, for the purpose of grinding circular points, a Hook joint is used to connect it with a driving pulley.

It may be proper at this point to offer a few observations, derived from experience, on the kind and quality of glass best suited to receive delicate lines. I have previously made some remarks before the Academy on what, for the want of a better term, was described as the *grain* of polished crown-glass. Subsequent observations have not entirely confirmed the views expressed at that time. Still, there does not seem to be much doubt but that certain kinds of glass are capable of receiving perfect lines only in one direction. When the lines are ruled at an angle with the general direction of the grain, the edges at once become serrated if they are very fine; whereas, if they are coarse, they either become enlarged throughout their whole length, the edges remaining smooth, or else they wholly break up, presenting a very ragged appearance. If the lines are as fine as 25,000 or 30,000 to the inch, this delicately serrated appearance can be detected at once; whereas, if the lines are coarse, several days may elapse before the tension by which the particles seems to be held together, is broken.

Two instances occurring in my own experience may serve to illustrate this action. In one, while I was examining a set of lines some days after they were ruled, I was fortunate in seeing two or three lines enlarge throughout their whole length. From being fine lines, they became, almost in an instant, very heavy lines, smooth, black, and of excellent quality every way. The action of breaking up was just slow enough to enable me to follow it. In the other, the lines had been ruled about two weeks; and for protection they were covered with microscopic glass, closely cemented to the surface. During my examination, the whole surface became completely broken up. Such was the force of the explosion that particles of glass $\frac{1}{1000}$ of an inch in length were driven a distance of $\frac{1}{100}$ of an inch. In fact, the debris covered the whole surface of the glass under examination. All the particles presented a curved appearance; and, with hardly an exception, the curvature was always in the same direction, On both of these plates lines were afterwards ruled in an opposite direction, but without noticeable results.

It may be said that this phenomenon was due to the peculiar action of the cutting crystal with respect to the surface of the glass; but in all subsequent experiments, in which similar but less striking results were noticed, lines were ruled in both directions at the same time, and under the same conditions. In the case of a particular importation of polished crown-glass from Chance & Sons, the evidence of grain is so marked and of such constant recurrence that all the large plates have been cut into slides 3 × 1 inches, in the direction indicated by the observations. In general, the direction of the grain can be detected at once by the appearance of lines as fine as 30,000 to the inch, while coarse lines may retain their initial character for several days.

The present indications are that the grain is only surface deep, and that it is the result of polishing in one direction. Common window glass seems to be wholly free from it. Nobert's lines are ruled on microscopic cover-glass about $\frac{1}{200}$ of an inch in thickness. The evidence of grain in this kind of glass is strong; but it is hardly decisive. In some specimens it is very marked, while in others it seems to be entirely wanting. Indeed, any conclusions on this subject must be regarded as only provisional, owing to the extreme difficulty of separating the action of the cutting crystal upon the glass from the effect due to the character of the glass itself. It is, however, safe to say that in certain kinds of glass the best results can only be obtained by ruling in a given direction.

In order to rule bands with lines separated by intervals, *e.g.* of $\frac{1}{60000}$ an inch, it is of course necessary to rule single lines whose width is less than this. Great precaution is requisite here, in order to avoid optical delusion. Every microscopist is familiar with the phenomenon of false lines. To avoid errors from this source, a few single lines are ruled between two heavy finding lines. They are then filled with graphite. This precaution is necessary in order to give both visibility and distinctness to the edges. If the lines are not filled, they may appear much finer than they really are; that is, the objective being in focus for the bottom of the furrow may fail to reveal abrasions of the surface on either side. The graphite of the New York Graphite Company will easily fill the finest line that can be ruled with a diamond.

In order to measure the width of the lines, the following plan is adopted as presenting some advantages over the usual method of estimating it by comparison with the known value of a given division in the eye-piece micrometer.

First, a single line is ruled, which in the eye-piece apparently exactly covers the line to be measured under the objective. A few trials will suffice for this purpose. Having found what weight must be applied to the diamond to produce such a line, the next step is to ascertain how many lines, exactly like this one, can be ruled within the space of $\frac{1}{200}$ of an inch with a minimum space between each line. This will also require a few trials. For example, if with a $\frac{1}{15}$ objective and a B ocular, the space $\frac{1}{1200}$ of an inch in the eye-piece corresponds to $\frac{1}{15200}$ of an inch under the objective, and if it is found that fifteen lines can be ruled within this space, then the width of the line under examination is $\frac{1}{15200} \times \frac{1}{15} = \frac{1}{228000}$ of an inch; a result which is obviously *within* the truth, especially if the line in the eye-piece is made a shade larger than the line under the objective. Tested in this way, the lines of Nobert's 19th band are about $\frac{1}{180000}$ of an inch in width. The photographs made by Dr. Woodward seem to give a little greater value. The finest lines I have succeeded in ruling are about $\frac{1}{160000}$ of an inch in width. These values are substantially the same as those given by Dr. Royston Pigott, as representing the ultimate limit of visibility under the microscope. The smallest angle at which an object can be distinctly seen is stated by him to be 6″, while other writers place it as high as 60″, or even 120″. Even the smallest value named is much too large. I will at any time undertake to rule a single line, $\frac{1}{30000}$ of an inch in breadth, which can be seen at the distance of seven inches from the eye. This corresponds to an angle of about 1″. In this case the line is filled with plumbago, but, if it is reflected from a silvered surface, it can be easily seen at the distance of eleven inches from the eye. Comparing minute particles of matter which can be *seen* under a Tolles $\frac{1}{10}$ objective with those which can be *measured*, in the way indicated above, there is every reason to suppose that the limit of visibility falls beyond $\frac{1}{400000}$ of an inch. It is quite possible that the conclusion reached by Sorby, that the microscope has already reached the limit of its power in *separating* lines whose distance apart is equal to one half of a wave length, may be found to be justified by future observations. It is certain that no lines *beyond* Nobert's 19th band have ever been resolved. The great difficulty in distinguishing true from spurious lines has caused more than one skilful microscopist to doubt whether the resolution has been *certainly* carried as far as that point. But that light is "of too coarse a nature" to enable us to see particles of matter as small as $\frac{1}{200000}$ of an inch, is a conclusion which can be refuted without the slightest difficulty.

How are Nobert's Finest Lines produced?

In trying to answer this question, I shall give the results of four distinct lines of investigation. Neither of these furnish conclusive evidence, but they are all suggestive of possibilities.

I. I have already stated that there is strong evidence that they are ruled with a diamond having a knife-edge. To this is added a fact derived from my own experience, and confirmed by a trial of several months; viz., *that when a diamond, having a polished knife-edge, is set slightly inclined to the direction of the lines ruled, its ruling qualities improve with use.* The diamond with which bands of 50,000 lines to the inch were first successfully ruled would at first barely rule 10,000. It was only after a service of several weeks, its position in the holder meanwhile remaining unchanged, that the highest limit named was reached. Four new diamonds have since been mounted with precisely the same result. It is not to be understood that this remark holds entirely true for heavy lines, such as are requisite for good diffraction plates. It is the experience of Rutherford and others, that one of the chief difficulties in producing such plates is the inability to find a diamond which will do its work equally well throughout the entire process of ruling. But when only very fine lines are desired, the longer the diamond is used, the greater the pressure which can be applied without increasing the size of the line. In this way the lines can be made much more uniform throughout their entire length, than when the diamond barely touches the surface. One can hardly say that the diamond sharpens itself by use, but there is some evidence that the wear is greater on the two faces than on the knife-edge.

When the diamond does its work perfectly, the cut, even of the finest line, produces a sharp singing sound. My ear has become so accustomed to this peculiar tone, that I can judge of the quality of the lines ruled almost as well by sound as by sight. In ruling the highest bands, this sound can be heard throughout the entire length of every line. It does not always have exactly the same character, however, being sometimes much sharper in tone than others.

II. From Mr. Herman, a successful diamond worker of New York, I learned a fact which was thought to be of sufficient importance to justify a somewhat difficult experiment. He stated to me that his experience had shown him that the only really hard points of a diamond are those where the line formed by the intersection of two faces terminate. His directions, therefore, were to grind the faces to a knife-edge, exercising great care to leave the natural line of intersection untouched as far as possible, and then to grind and polish a face nearly at right angles to this line, stopping just at its extremity. He assured me that the success of the experiment would depend entirely upon neither falling short or going beyond this point. Only one diamond has been successfully prepared in this way, and even in this case it is not quite certain that this requirement has been met. Its performance is sufficiently good to warrant further experiments.

III. I am indebted to Mr. D. C. Chapman, of New York, for a third method of preparing a ruling diamond. It is allowed by all familiar with the subject, that the natural face of a crystal is harder than any surface formed by breaking the stone into chance fragments. By splitting a stone in the direction of a cleavage-plane, forming an angle of about 40° with this natural face, an exceedingly sharp knife-edge may be formed, possessing excellent ruling qualities. Moreover, in ruling heavy lines for diffraction plates, the cutting-edge retains its form for a long time. In setting the diamond for ruling, the natural face should be slightly inclined to the surface to be ruled. The Brazilian "bort" seems to give the best and most durable cutting-edge. With a diamond prepared in this way, the line formed by the intersecting faces being about $\frac{1}{16}$ of an inch in length, I find little trouble in ruling from 60,000 to 80,000 lines to the inch.

IV. A few months since Mr. R. C. Greenleaf, of Boston, placed in my hands a Nobert plate which had been entirely spoiled by the introduction of some kind of fluid between the ruled glass and the slide on which it was mounted. Mr. Greenleaf requested me to undertake the restoration of this plate, kindly offering to assume all the risk of failure. The cover, which had been imperfectly cemented to the slide with something like opal cement, resisted every attempt at loosening. As a last resort, two pieces, about $\frac{1}{10}$ of an inch square, were cut with a diamond from the centre of the cover glass. After several trials, one of these pieces was cleaned and remounted without material injury to any of the bands. The 19th is quite as easily revolved as in other Nobert plates.

The other piece, being less perfect, was made the subject of a somewhat careful study. Among other experiments, an attempt was made to fill the lines with graphite; but it was found impossible to do so. Even the coarsest lines would not receive and hold it. As I had never before found any difficulty in filling lines either coarse or fine, this result, so entirely unexpected, was noted down as one of which no explanation could be given at that time.

A few weeks afterwards, I succeeded in reducing a black carbon to a knife-edge. Upon an examination of the first lines ruled with it, two facts at once engaged my attention. First, the lines were finer and smoother than any I had ever before ruled. They possessed that quality of glossy blackness which characterizes nearly all of Nobert's lines. Moreover, they seemed to stand out more boldly in perspective than lines ruled with the ordinary diamond. Every one who has made a study of Nobert's diffraction lines will at once recognize this boldness of perspective as a characteristic feature. Secondly, I was equally surprised to find that the lines *would not receive and hold graphite.*

As these results were confirmed by further observations, it did not seem too much to say that possibly the secret of Nobert's success might consist in his use of a prepared carbon. The natural stone is entirely unfit for ruling purposes.

But it appeared subsequently that this conclusion was quite too hastily formed, as far as the capability of receiving graphite is concerned. During all these observations, the position of the diamond in its holder remained unchanged; but it was afterwards found that, by giving it a certain inclination with respect to the surface of the ruled plate, it was possible to rule lines, both coarse and fine, which would receive the graphite in the most perfect manner. In general, however, lines ruled with a carbon will take the plumbago perfectly but once. If they are filled and the surface of the glass is afterwards cleaned by rubbing, it is not possible to fill them equally well again. As the filling is not disturbed by mounting in balsam, the better way is to clean the glass thoroughly before ruling, and then mount permanently after the first filling.

Though the carbon is reduced so perfectly to a true knife-edge that the intersection of the two faces appear as a line when examined with an eye-piece of high magnifying power, it is apparent, nevertheless, that the cutting-edge is composed of distinct and separate crystals; for in many cases two lines have been ruled at the same time. Generally one is much coarser than the other. Indeed, by regulating the pressure, companion lines can be ruled so fine that it is impossible to see them until they are filled. The setting of the diamond to rule lines of a given kind and quality is simply a question of time and patience. In one hundred trials, perhaps two or three may give lines which will receive plumbago, four or five may give double lines, and one or two may give lines of great delicacy. Great care is necessary in the preservation of the minute cutting crystal when once found. Notwithstanding the most careful manipulation, it often gives way without visible cause. In several instances, I have been able to locate the exact point where it was destroyed.

In general, the best results have been obtained with the prepared carbon. It is, however, somewhat capricious in its action. The labor of preparation is also much greater than with the African or the Brazilian diamond. The process of grinding occupies from five to ten days. That it is much harder than any other kind of diamond is conclusively shown by the fact that one specimen in my possession has been used in shaping a jewel weighing 180 carats, with only a trifling abrasion of its surface.

In conclusion I ought to say, in explanation of the somewhat incomplete and fragmentary character of this investigation, that it has been the gradual outgrowth of experiments undertaken for a different purpose. Indeed, whatever has been accomplished thus far may be said to be the result of an unsuccessful search after a spider that would spin a web suitable for the meridian circle of Harvard College Observatory. Failing to find suitable "spider lines," an effort was made to produce on glass, lines of the desired quality and size. This was finally accomplished by etching with hydrofluoric fumes; the lines having been first ruled in a coating formed by dissolving white wax in gasolene, and uniting the solution by emulsion with liquid gelatine. The coating thus formed will receive lines as fine as 10,000 to the inch, while its protecting qualities are sufficient to withstand very strong fumes. The subsequent experiments detailed in this paper have occupied my attention from time to time during the past three years, when not engaged in my regular duties as Assistant in the Observatory.